Responses to Reviewer 2:

We thank the reviewer for a careful reading of the manuscript and for helpful comments.

Specific comments

1. Abstract: "Sysyphusian" is not a word. Perhaps the authors meant "Sisyphean"? But that term refers to a task that cannot be completed, which does not apply here because (a) there is no agent performing a task here, and (b) the system does completely recover from the open cellular convection. I would use a term that is consistent with the study's findings, instead.

The reviewer is correct. We should have used Sisyphean. Regarding whether the task can indeed be completed depends on the circumstances (e.g., in Fig. 7c the trajectory in LWP; TKE space is not closed and so strictly speaking full recovery has not occurred.). The term was intended to contrast between the runaway effect for the closed to open transition and the relative difficulty of recovery. Nevertheless, given a desire to appeal to a broad readership that may not have been exposed to these narratives, we remove the term.

2. p 5556, l 22: The claim that avoiding aerosol entirely and instead directly controlling cloud droplet concentrations, "allows a more direct assessment of the importance of the rates of aerosol removal and replenishment" does not make sense and needs clarification. How is it that bypassing aerosol completely allows for assessment of aerosol sinks and sources (which are never assessed)? This sentence would make sense if "does not" were inserted before "allows".

The reviewer is correct. "allows" is changed to "avoids"

3. p 5557, l 5: I would say "SAM solves the anelastic equations" or so rather than the confusing statement that "SAM is an anelastic system". Note that it also provides an option to solve the Boussinesq equations for shallow convection in LES mode according to the paper cited.

Changed as suggested.

4. p 5557 l 16: "Grid size" should be replaced with "Grid spacing" or so and "smaller...grids" should be replaced with "finer...grids" or so, since the term "small grid" and "grid size" describe the size of a grid, not mesh refinement.

Changed as suggested

5. p. 5558, l 9: "Rainrate" is not a word. Also, is the rain rate defined at the surface or cloud base or what?

We suspect that different journals have their respective spelling preferences and leave this to the technical editing phase.

The rainrate in the predator-prey model is a system rainrate that is not vertically resolved. This is now clarified.

6. p. 5559, l 17: The notation "m g^-1" means "meters per gram" where the authors certainly intended "mg^-1", meaning "per milligram". This notational error pervades the text and figure captions.

Thank you for catching this. The original used mg^-1 and the space between m and g was added during the ACPD typesetting stage.

7. p 5560, l 5: Given that LWP includes cloud water and rainwater, the modifier "cloud" before "liquid water" should be omitted.

Actually the liquid water path shown is just the cloud part, which is why the modifier was used.

8. p 5560, l 16: The term "commensurate" does not fit here. "Incommensurate" would be closer to what is being described, but I'd rephrase and pick another term entirely.

We are not sure why because the larger the imposed reduction in N, the larger the asymmetry. Therefore "commensurate" is appropriate.

9. p. 5560, l 23: "Cloud formation is CCN-limited" seems odd here, since there are no CCN in the simulations and clouds apparently form just fine in the simulations even at extremely low cloud droplet concentrations of 5/mg. Some rephrasing or omission is needed.

The text is rephrased. We note that at these very low N, the interacting outflows seem to be important for cloud maintenance.

10. p 5560, l 25: It is stated that R goes as LWP^1.5/sqrt(N) as if that were some universally-accepted relationship. It's not. It might be interesting to show how the results here compare with that relationship, though.

Agreed. Our intent was to use one common expression but we know generalize.

11. p. 5560, l 27: I would define ambiguous terms upon their first mention, such as f_c, z_i, and z_b, which can be defined in many ways. I would provide the definitions used in the analysis here.

The criteria used for these calculations are now defined.

12. p. 5561, l 18: The "left panel" is referred to but there are three of them. Perhaps "left column of panels"?

Changed to "left column".

13. p. 5561, l 19: It would be helpful to note after stating that the cloud layer warms that one can figure that out by noticing that theta_l is steady while q_l decreases, which implies that theta must have increased.

Noted as suggested.

14. p. 5561, l 21: The interpretation seems to imply that the 9.5 g/kg isosurface marks the top of the near-surface layer. The thinking is unclear.

It was not our intent to claim that the 9.5 g/kg isosurface marks the top of the near surface layer. We have now clarified the text.

15. p. 5562, l 6: "Largescale" is not a word.

We leave it to the technical editing phase to sort this out.

16. The first paragraph of Section 3.2, which is attempt to explain the relationship between LWP, precipitation, and TKE, could use a good bit more attention and clarification so that it becomes clear and that physical understanding is effectively conveyed.

This is now done in the revised version.

17. p. 5563, l 1: Conceptual elements are missing from the assertion that precipitation reaching the surface cools the surface and warms the cloud layer, because the statement does not make sense as presented.

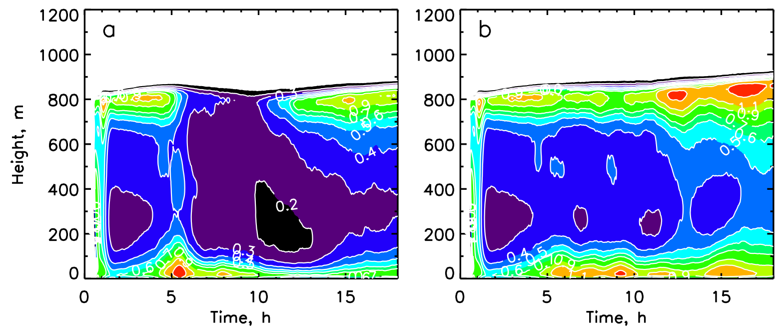
The text is rewritten to explain this more fully.

18. p. 5563, l 8: When stating "LWP drives production of TKE" it would be helpful to note that there is a positive feedback at work in which TKE also supports LWP.

Added as suggested.

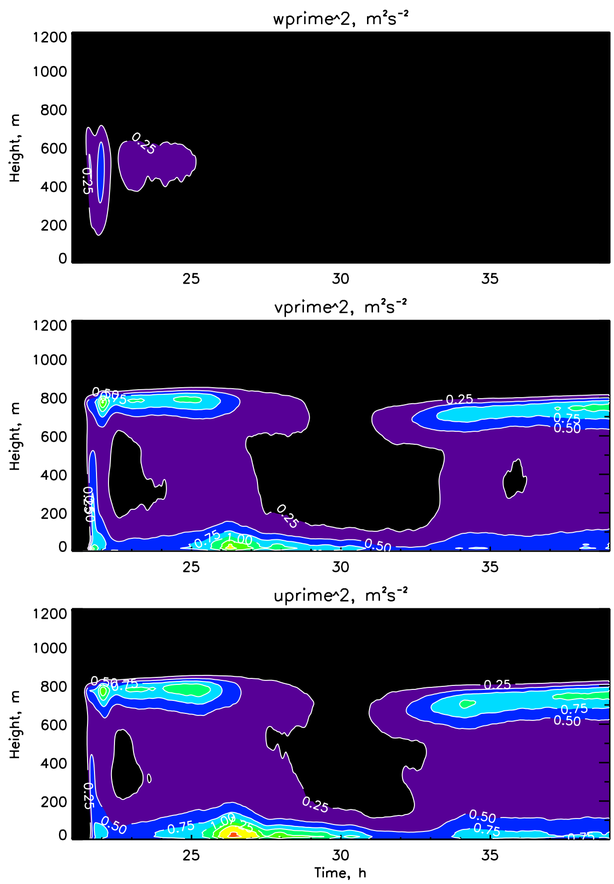
19. p. 5563, l 10: It would be helpful to explain why there is a roughly 1-h delay between LWP decreasing and the drop in TKE.

We now add an analysis of the TKE. Below is an example of the mean TKE profile as a function of time for the strong N reduction (90→5/mg) (left) and the weak reduction (90→35/mg) (right). One sees a clear shift in the TKE from the cloud to the surface associated with the transition from closed to open-cell state. The surface TKE is stronger for the more strongly precipitating case. It is this surface peak that contributes to boundary layer TKE and accounts for the delay.



The figures below break the TKE into its contributions and show how the cold-pools contribute to TKE.
The initial strong rain event drives strong surface TKE in the outflow, which slowly decays with time.

Figure below: TKE broken into components: This analysis pertains to the analysis in current Fig. 7.
All have the same color scale.



20. p. 5553, l 17: It is stated that the phase space trajectory "nicely" shows a limit cycle, but it does not. The very essence of a limit cycle is that a trajectory is closed, but the trajectory that is shown is open. High concepts are great, in principle, but readers may *question their value when casual inspection reveals that they don't actually fit the evidence provided.*

The reviewer is correct. We change the text to ".. nicely demonstrated as a plot in LWP, TKE phase space".

21. Section 3.2.2: It is unclear why the authors choose to increase the surface sensible heat flux with a goal of accelerating recovery. Increased sensible heat flux should reduce the relative humidity of the boundary layer and instead of generating thicker cloud, as mentioned on line 13, should generate thinner cloud, no? Or another angle – the authors seem to understand that increased radiative cooling is needed for the system to recover. Increased radiative cooling is removal of sensible heat from the system, working in the opposite direction of a "strong influx of energy" mentioned on line 8. So it seems to me that the entire notion of attempting to accelerate recovery by adding sensible heat is backwards, and it should only serve to slow down recovery.

First, as stated in the text, both sensible <u>and</u> latent heat fluxes are increased (maintaining the same Bowen ratio). Therefore increasing the fluxes will not necessarily result in thinner cloud.
Second, adding surface heating drives stronger turbulence as does increasing cloud top cooling.
Our point was to show that an added source of heat and moisture (dynamical forcing) could aid the recovery. We understand that this was not clearly laid out and have revised the text.

As an extra check, we also reran this simulation with a doubling of only the latent heat fluxes and achieved essentially the same result, since LH is roughly 3x larger than SH (current Fig. 4). There is one distinct difference however: when doubling only LH, the period of recovery to closed cell state is characterized by much more frequent shallow convection with low cloud base.

22. p. 5564, l 15: It is stated that "higher SH and LH are typically as [sic] drivers of open-cell formation" but aren't changes in sensible and latent heat fluxes the result of other changes associated with open-cell circulations, rather than drivers? Otherwise, open cells could be generated by simply increasing SH and LH fluxes. Can they? Furthermore, if higher latent heat fluxes are drivers of open-cell formation, how does that conform with open cells being associated with lower latent heat fluxes in fig 4b?

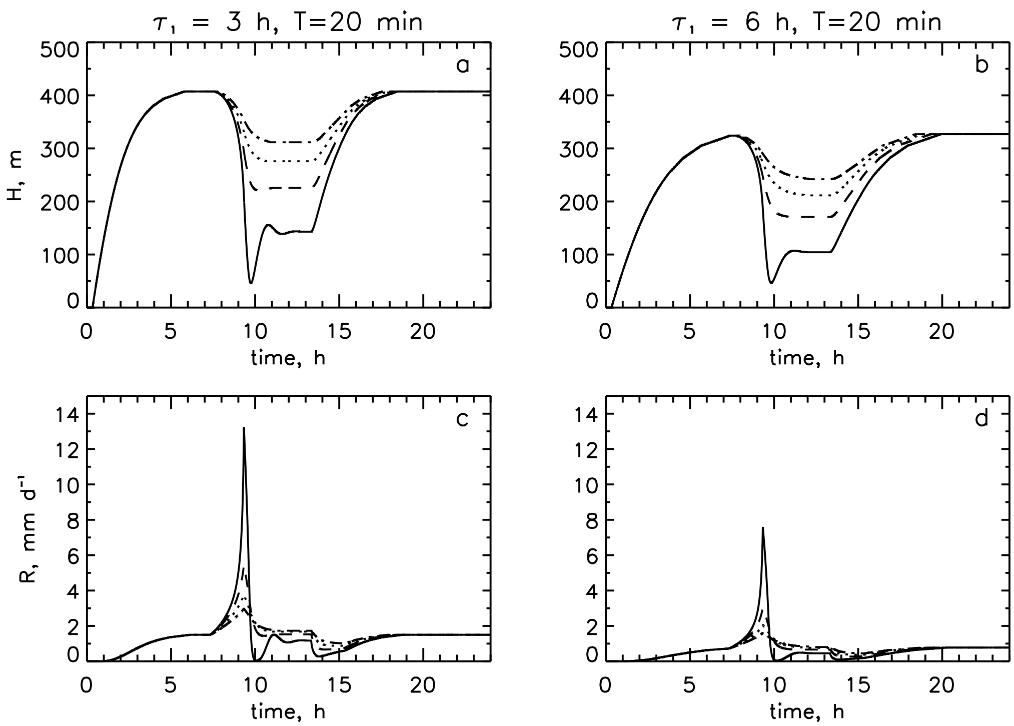
We agree that this section was not clearly laid out and it is now revised. Indeed, the cold pools associated with the raining period reduce LH and increase SH.

23. The foregoing issues regarding surface heat fluxes also appear in the abstract and conclusions.

Changes are made.

24. Section 4.1 contributes no understanding to this reader and the manuscript would benefit from omitting it. Either that or it needs to be fleshed out and tied into the rest of the study in a manner that adds value and conveys understanding.

The revised manuscript has a more thorough investigation of the predator-prey response. We now show results for the same range of N(t) as in the CRM results and show similar recovery characteristics. We contrast two time scales for recovery (3 h and 6 h) and show the impact on recovery. Note that both of these timescales produce reasonable rainrates (1-2 mm/day). We also discuss how the delay terms in the equations create an inherent asymmetry in the system.



25. The rain rates for the predator-prey model seen in fig 9 are greater than those for the CRM by orders of magnitude, yet this is never even noted, let alone remarked upon. Seems like the dynamic regime of the predator-prey model is very different from that of the CRM simulations. Given such an adjustable model, the authors should either adjust it to be consistent with the CRM simulations or explain why that is impossible.

The revised manuscript presents predator-prey results that have similar rainrates. Nevertheless, we do stress that the goal of the simple system is to mimic behavior rather than exact values.

26. Section 4.2: The authors' understanding of the purpose of Beer's law longwave parameterization does not make sense to me. The reason it is used in model *intercomparisons is to reduce possible sources of discrepancy between models, which typically use different radiative transfer schemes. The notion implied here that the Beer's law treatment represents an alternative treatment to real radiative transfer is very much off-target. The Beer's law treatment provides a small number of adjustable parameters that Larson et al. (2007) have shown allow it to reproduce the heating rates from real radiative transfer models. So if the authors find that the Beer's law formulation does not produce heating rates that are comparable to those with their radiative transfer model, that just shows that the authors failed to tune the adjustable parameters so that the rates are comparable. Used properly (which means tuning the adjustable parameters to reasonably match the heating rates given the conditions input to a real radiative transfer model), the only disadvantage of a Beer's law formulation in this context is that it is not set up to readily compute solar heating. It should be stated that such an extension would not be difficult, and the reasoning for not doing so provided.*

We agree with the reviewer. The text is now changed to reflect this point. We do note that it is fortuitous that the (un-tuned) Beer's Law treatment generates weaker cooling than the RRTM because it allows us investigate the role of the radiative cooling in the recovery.

27. The authors' claim that there may be some biases for the Beer's law formulation for broken clouds, even though it is being used with the independent column approximation. But RRTM is also being used with the same approximation. Why would there be any bias if both approaches are using the same treatment to treat horizontal heterogeneity?

The reviewer is correct. The text is changed.

28. Instead of, or in addition to, stating the specific humidity used for the free troposphere in RRTM, it would be helpful to provide the overlying column of water vapor, which is more physically relevant.

This is now added to the figure caption along with the details of the profile.

29. Appendix: "Grid size" should be "grid spacing" or so. Also, it should be stated whether or not the domain size is fixed for these tests.

Changed and clarified as suggested

30. Panel labels are far too small in fig 2.

Labels are increased as suggested.

31. The surface precipitation rate shown in fig 3 is about a factor of five smaller than the average value measured in the open-cells for this case. This discrepancy should be noted and the implications discussed.

The original figure showed the domain-average rainrate. The revised figure shows the rainrate averaged over precipitating areas with a threshold rainrate of 0.1 mm/day. This accounts for the factor of 5 identified by the reviewer. Both timeseries are of interest but we now show the conditionally averaged ones.

32. There is a units problem in the equation provided in the fig 6 caption.

The text is changed to clarify the units.

33. The "domain and boundary-layer average" mentioned in the fig 7 caption is confusing. Surely the domain is deeper than the boundary layer, so this description does not make sense.

The average was done over the boundary layer, both horizontally and vertically. This is now clarified.

34. The legend, which appears to show grid sizes (numbers without units would seem to indicated that what is referred to is the number of grid cells), evidently conflicts with the description in the main text. A more complete figure caption might help.

This is now clarified and the legend and caption are changed to make it clear that grid spacings/lengths are in meters.